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FROM THE PRESIDENT OF INDUSTRIAL RESEARCH INSTITUTE, INC.

It is gratifying to me that the activities of various IRI committees, our subsequent conversations with officials of Interscience Publishers, and official actions of both the Institute and Interscience have resulted in the founding of a new professional journal devoted to research management.

The publication of such a journal is a logical implementation of the purposes which led to the formation of IRI in 1938 under the auspices of the National Research Council and its subsequent establishment as a nonprofit membership corporation. From modest beginnings the Institute has grown until today its member companies are responsible for the conduct and management of a large portion of all the industrial research and development activity being carried on in the United States.

Throughout its existence the Institute has held steadfastly to its purposes as stated at the time of its organization. Namely:

(1) To promote, through the cooperative efforts of its members, improved, economical and effective techniques of organization, administration and operation of industrial research.
(2) To develop and disseminate information as to the organization, administration and operation of industrial research.
(3) To stimulate and develop an understanding of research as a force in economic, industrial and social activities.
(4) To promote high standards in the field of industrial research.

This new journal, *Research Management*, roundly complements the other current activities of IRI in the furtherance of these purposes. *Research Management*, designed to reach both Institute and

non-Institute organizations engaging in research and development, in my estimation will be a powerful force in the continuing development of proficiency in research management.

Research Management is fortunate to have at the outset such a distinguished Advisory Editorial Board. Its members will play an important role in the future of this journal. These board members are research administrators with proven records of performance. Their counsel, freely and generously given, is priceless in the true sense of the word. My sincere thanks to the Advisory Editorial Board and to our Editor, Charles M. Burrill for their participation in this venture. I am confident of the values to accrue from their efforts.

THOMAS H. VAUGHN

FROM THE EDITOR

Why another publication? Isn't there already enough for the research administrator to read? Maybe so, but it is not easy for him to find what he wants. There has been no one place to turn for the latest ideas regarding research management discussed from the research administrator's point of view. It is our ambition for *Research Management* that it may come to fill such a place.

We seek to serve all those who are concerned with research management, whether in industry, in government, or in the universities. In the selection of subjects and manner of presentation, we shall have in mind the research worker who desires to understand the management side of his work and to prepare himself for administrative responsibilities as well as those already in administrative positions.

We are beginning by publishing selected papers that have been presented at meetings of the Industrial Research Institute. However, we shall welcome contributions within our selected field from other sources. If our readers will help by submitting manuscripts or suggesting subjects, and by sending comments in "Letters to the Editor," we shall the more surely reach our goal.

C.M.B.

ABOUT THIS ISSUE

In this issue we are publishing four papers presented at the October 1957 meeting of the Industrial Research Institute at Pittsburgh, Pennsylvania. These papers were part of an all-day session devoted largely to case histories of research. These case histories gave intimate pictures of the essential steps in the progress of typical research projects, for the purpose of illuminating the research organization and the research management decisions required.

ROBERT G. CHOLLAR, Vice President, Research, of the National Cash Register Company, and his associates, GALEN J. WILSON, Assistant to the Vice President, Research, and B. K. GREEN, Director of Chemical Research, all of Dayton, Ohio, outline the development of a new product called NCR (No Carbon Required) Paper. All three authors are chemists.

The development of another new product, the Fluorex X-ray image amplifier, is reviewed by J. W. COLTMAN, Manager, Electronics and Nuclear Physics Department, of the Westinghouse Research Laboratories at Pittsburgh.

Statistical techniques in the design of experiments are discussed by E. M. MCCOLM, Research Chemist with the United States Rubber Company at their central research laboratories at Wayne, New Jersey. The discussion is in colloquial vein, nonmathematical, and for both the research worker and the administrator. It will be easier reading than you think, and the conclusion will, we believe, be clear and relevant.

In the concluding paper, Dr. R. S. ROSE, JR., Manager of Administrative and Technical Services, Chemical Research Department, Atlas Powder Company, Wilmington, Delaware, describes a pioneering attempt to upgrade literature search to full creative research status by staffing an Information Research group with creative, technically trained personnel. We think you will find interesting his frank evaluation of the problem. Dr. Rose is a chemist.

CREATIVITY TECHNIQUES IN ACTION

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INTRODUCTION

Creative research techniques have been used to produce a successful product known as NCR (No Carbon Required) Paper. This presentation outlines, by using the development of NCR Paper as an example, significant factors that we believe can be effective tools in stimulating creativity and in forming a productive, professional research organization.

The subject has three parts. It is our observation that creative techniques are different in scope and text when viewed and utilized by staff, line, and top management groups. The first part concerns techniques developed and implemented at staff level. The second part is based upon line concepts and utilization of different techniques. The conclusion may be regarded as the third part, representing the manner in which top management uses the techniques and results of techniques in the over-all integration of products.

We shall avoid unnecessary reference to the conventional techniques that are easily recognized and frequently used, and we

shall assume that the important factors of good communications, title structure, salary, rewards, facilities, etc., are inherent in any research program where stimulation of creativity is an objective. There are minimum standards that can be ascribed to each of these areas, below which there is insignificant return on investment. The National Cash Register (N.C.R.) research operation more often occupies "The Middle Way" and has never been forced into excesses of salaries, titles, or extravagant facilities. We are competitive in salaries, and we have new and complete facilities in our several domestic research operations.

The development of new products is a carefully organized procedure at N.C.R. A Future Demands Committee sits with the Board Chairman and President in frequent review of changing market requirements and related market analyses. A Market Research Department functions in staff position to the President.

Product Development is an additional department, comprised in large part of senior salesmen who have had "grass roots" training and experience in the sales combat field. They work "hand and glove" with the Research and Engineering Divisions to interpret field needs and assist in evaluating Research and Engineering developments that are closely related to product lines.

In research we have a Project Specifications group and an Applications Research group to aid in project orientation, liaison problems, integration of programs, and communications between research groups and between Research and Engineering Divisions. Several functions of these groups are related to products that are not direct-line products of N.C.R. These are the diversified products that stem from exploratory research, or are sideline developments of research projects.

Within the research groups themselves, additional techniques are used to integrate the various scientific and engineering efforts. For example, in one group concerned with fundamental chemical research, we have on the staff: organic chemists, physical chemists, biochemists, physicists, electrical engineers, and mechanical engineers. Men of these various fields are working together every

day on overlapping problems. Such close contact permits the assimilation of knowledge in single individuals. It is only when all phases of a problem are well understood by everyone that effective progress can be made in integrated areas.

NCR Paper Defined

NCR Paper is a unique system, designed to eliminate carbon in manifolding forms. It is used in many different types of business forms such as sales orders, bills of lading and receipts. When a mark is printed or written on the top sheet, a duplicate mark appears on each succeeding sheet in a collated form. The entire form is clean and colorless, for no carbon coating materials are required.

NCR Paper is a multi-million dollar a year business. It was introduced in 1954 and is currently a growing segment of the one-time carbon forms market.

Part I: Techniques at the Staff Level

Dr. Morris Stein of the University of Chicago defines creativity as "that process which results in a novel work that is accepted as useful or satisfying by a group at some point in time."

The following subject areas are those generated and utilized at staff level to bring about this "novel product that has been accepted by a large group at this point in time."

1. Problem Orientation
2. A Research Referee
3. Progress Evaluation
4. Working Combinations
5. Consulting Programs
6. Communications

Problem Orientation

A most important initial factor was the definition of the problem, and the orientation of research personnel to that problem.

Improvement of output media in terms of printing quality had long been an Engineering and Research objective at N.C.R. The problems of ribbon printing and attendant mechanical ribbon reversing mechanisms were well known. With these points in mind, the Research objective was established to incorporate within a paper all of the components required for printing, thus eliminating bothersome ribbons and complex mechanisms.

In the beginning, few specifications of the desired end-product were available to guide research efforts. This status presented the research people with loosely defined goals and clouded objectives. Specific product goals had to be communicated to the researcher. In addition, it was necessary to instill in the researcher an awareness of management objectives, and an understanding of the company products in which NCR Paper could play a part.

An extensive orientation program to acquaint researchers with the function of business machines was established to develop an understanding of N.C.R. products. All key research people on the project spent several weeks visiting various divisions and departments in our manufacturing operations. They saw for themselves the business machines which would use NCR Paper, if and when it was developed. They talked with the skilled assemblers and inspectors to learn about printing mechanisms and the problems of carbon paper used in business machines. The information gathered from these men, who knew and understood the meaning of field performance requirements, filled in blind spots about product requirements for the research people.

Another one of the objectives in the original problem was to develop a paper system that would eliminate the need for carbon paper in N.C.R. business machines. However, as early feasibility studies were completed, a broadening of market horizons became apparent. We soon realized that here was a possible product with

much greater dollar potential than had been anticipated. The significance of this became increasingly evident to the research people and they responded with new enthusiasm.

A Research Referee

At this point, we called in our Applications Research group to be a part of the team effort. Their responsibility was to determine the market potential and to understand and translate to research people the technical requirements of a unique copy paper that would eliminate carbon.

This group reported that we were in a market with a million dollar potential. Furthermore, they understood what properties and characteristics this new paper product must possess.

Another interesting and important responsibility was added to the Applications Research Department. They were assigned to evaluate research samples as they were produced in the laboratory. Perhaps the greatest value of the Applications group was the constant communication of field problems and the characteristics of the problem to the researcher. This stimulated the research man to constantly evaluate, orient, or even change the approach to his problem.

Such an organizational setup as we have in the Applications Research Department is not without its problems. There is a sensitive balance to maintain between the research man who is enthused with his own progress, and the observer across the bench who, it seems, is constantly calling attention to the negative points. Here is where it pays to have the evaluation group comprised of technical personnel capable of understanding the problems and difficulties of the scientist at the bench. This is also where understanding leadership plays a vital role. To obtain the proper balance of critical evaluation and yet not frustrate creativity was a delicate operation in research administration.

Through integration of the efforts of the Applications and Research groups, original objectives were modified to provide a

method for producing one-time carbon business forms without the use of carbon paper or colored materials of an ink-like nature. These decisions were based on new scientific principles evolved in combination with economic and market survey data. The use of these new scientific principles is rapidly progressing so that within the foreseeable future, NCR Paper will capture a significant share of the one-time carbon forms market.

Progress Evaluation

Not all problems were resolved with the establishment of an Applications Research group. There remained a weak communication link between Research laboratories, the Applications group, and Research management. Each person was handling his assignment, but it was without proper integration into other equally important project phases. Communication was somewhat haphazard. Individual and isolated discussions failed to provide a true perspective of the bench problem and its relation to the ultimate product requirements.

To provide an answer to this lack of integration, we utilized the "Weekly Review Session." Laboratory progress was developing rapidly and significant strides in a week's work were ample bases for up-dating discussions. We drew every professional man into these meetings. Our consultants were asked to participate along with Applications Research, Management, and the researchers and pilot plant operators. In these meetings we probed every aspect of the technical problems. The Applications men gave factual and critical reviews of the samples they evaluated in the field and in their own testing facilities. The criticisms were not always accepted by the research people as helpful hints. Similarly, the urging of management for target dates was often regarded as "just not scientific" and "you can't do research that way."

But the team effort yielded results. We have found that a sense of urgency is not a deterrent to creativity when the problem is well defined, the leadership is sincere and impartial, and a realistic

goal can be discerned. Under these conditions there is group harmony that stems from understanding—not only understanding of technical problems, but of "where we are going" and "how" and "when."

Working Combinations

In the weekly reviews it was sometimes evident that certain technical knowledge was not available in the assigned research group. However, it was very often found that the information was available in one of the other laboratories within N.C.R. In these instances we brought personnel from other laboratories to throw their technical knowledge into the arena. They sat in the weekly reviews and formed a vital segment of the total team.

It is apparent that the team approach was a major technique from project inception. Regular intra-divisional meetings were used to keep the broad scope of each project in the focus of personnel in all laboratories.

Chemical engineers, colloid chemists, organic synthesis specialists, pilot plant operators, etc., were kept familiar with basic project problems regardless of their particular laboratory affiliation. This familiarity provided a logical basis for enlisting the aid of outlying laboratories at any point in time.

Consulting Programs

Outside research institutes or universities were used on at least four different problems. Some of these have been successful, and others have merely helped to determine the magnitude of the problem requiring solution.

Problem orientation and the repeated need for problem reorientation by the consultant, together with geographic limitations, has proved to be a formidable barrier to the effective use of consulting organizations. The sponsor has the responsibility of imparting to the consultant both background experience and intimate

knowledge of end uses. These have proved to be very difficult barriers, so that our chief success in the use of consultants has been where the objectives are very specific, and where geographic conditions are such that the consultant is able to attend frequent meetings for re-orientation purposes.

In particular, we have been unsuccessful where the problem was of a broad, creative exploratory nature.

Let me illustrate this point with a phase of the NCR Paper problem.

It was clear from the start of the project that microscopic capsules containing the marking fluid would be the ideal solution to the problem. It was also clear that if a liquid could be obtained discontinuously dispersed in a solid, this would be a useful system. Textbooks on colloid chemistry briefly mentioned such systems to round out a system of classifications, but no synthetic or commercial processes were described. The literature was likewise devoid of such systems. However, it was simple enough to visualize what was required.

To produce such a system was, of course, a large order, as we were often reminded when considering the project with consultants. Geographical barriers, as well as lack of orientation and re-orientation, were contributing causes of failure. The problem was finally solved in our own laboratory.

Communication

Real team play is the result of many subtle influences on the individual. We secured team play by recognizing a basic human requirement for dignity in work and recognition of achievement.

In this area our communication lines from top to bottom were extremely short. Regular and informal trips through the laboratory by supervision kept management's finger on the pulse of progress. The up-dating to supervision was worthwhile, but it was a second-order benefit—the real reward was in having the research man at the bench tell his own story about accomplish-

ments and know that management knew about him. It was not unusual to see our Company President in the laboratory, following a call by the Vice President of Research, listening and looking with the man who had punched out a problem solution only the day before. It is repeated again—this technique pays off.

In the foregoing section, we have described techniques that were developed as deliberate organizational strategy. In addition, there are those influences on motivation that are far more subtle and which cannot be produced by edict. They develop as a result of attitudes at every management level, and become a vital part of the philosophy of every research man.

Part II: Techniques at the Line Level

Open Project Time

So far, creative research techniques generated on a staff level have been discussed. Of equal importance are those which are utilized on the line level. The most important technique for use on the line level has been "Open Project Time." In this technique the researcher is given freedom to investigate any solution to a problem which is compatible with company goals.

The NCR Paper project received its first impetus through such project time. The primary project assignment was to improve the quality of one-time carbon coating material. There was, however, open project time limited only to improving printing in business machines. In this free time a principle was discovered and demonstrated that aroused enough interest to establish a more definite research project, which culminated in NCR Paper. Many of our more original research projects have been initiated by this technique.

Open project time also has a very important bearing on morale, and is an instrument for building morale. It permits self-expression and provides opportunity. More specifically, it has been useful to help Management guide personnel to positions in

the organization where they are best able to perform effectively. This adds to the well-being and morale of the individual and has important effects on group morale.

The advantages far outweigh the disadvantages. Productivity in research is measured not just in man-hours. Morale and enthusiasm created by open project time is believed to be far more valuable than the time itself in terms of creative productivity.

Exploratory Research

Open project time is often used for exploratory research. Exploratory research is a useful technique or method for introducing originality into a project. It also provides the basic tools for building a creative research process in a broader, more applied sense. Two basic principles were keys to the NCR Paper program. These are: (*1*) New color reaction systems, and (*2*) new principles of separating reactive materials.

Progress in the basic science of these two areas has served not only for the foundation on which the NCR Paper project was built, but has opened up many additional new applications and research projects.

These two areas are important for a clear understanding of how creative techniques have been applied to the NCR Paper project.

The requirements for a color reaction suitable for use in NCR Paper were very stringent. The most important properties required were:

1. High degree of stability in the colorless form.
2. High degree of stability in the colored form; namely, good fade resistance.
3. A very high degree of intensity.
4. High reaction speed.
5. Nonvolatility.
6. Solubility in inert stable solvents.

7. Nonreactivity with ordinary materials; *i.e.*, reactive only with specially treated paper (cleanliness).

It proved to be difficult to find color reactions that satisfied all these requirements. It had long been known that certain colorless dye derivatives in oil solutions reverted to their colored forms upon contacting suitable solid surfaces. The mechanisms of these reactions were not very well understood, and all known reactions came far from fulfilling the requirements. It has not been possible to incorporate all the requirements in a single compound. A combination of two compounds was tailored to fit all of the requirements; one of these compounds had never before been synthesized. Its properties were predicted before it was made, and today it is the compound responsible for the blue color in NCR Paper forms. Certain clay materials were found to provide reactive solid surfaces for converting the colorless organic materials to their colored forms.

The knowledge gained in the exploratory research on color reaction phenomena is proving to have far wider application than just for NCR Paper.

The second important key to the NCR Paper system was how to place the oil solution in a thin paper coating without having it slowly bleed out and discolor and ruin the system. This was the most difficult single problem of the entire NCR Paper system.

The solution to the problem was accomplished by developing a process for making microscopic oil-containing capsules automatically by the billions. These capsules were so perfectly formed that it was possible to place gasoline-like solvents inside of them and heat the capsules above the boiling point of the solvent without any loss. Thus, in the NCR Paper coatings today, the oil solution can only escape by rupture of the capsular structure. The product has unlimited shelf life even under adverse conditions of temperature and humidity. We call the coating based on this capsular principle the "closed structure coating." Volatile oils cannot volatilize, soluble oils cannot be extracted, and reactive oils cannot react until the capsules are mechanically ruptured.

Independent Evaluation Group

The technique of establishing an Applications Research group as a separate entity from the Research group was important in carrying the original results of exploratory work through to a practical product. This group actively identified and defined the problem parameters. It was the accurate definitions of these problems, together with an evaluation of priority, that were fed back to the Research group. It was also the accurate definition of weaknesses in the systems, as they existed, that stressed the need for further exploratory research, and helped to guide research effort in the right direction.

Evening Discussions

The researchers themselves have initiated a practice which illustrates the morale and environment that prevails; this practice has been conducive to creative work. This comprises evening discussions at the homes of researchers where attendance is by invitation. The meetings are semi-social and provide opportunity for intellectual recreation. This is a result of the use of creative techniques rather than a creative technique, but once in operation, it has had a powerful influence on productive creative research effort.

Many discussions have revolved around the mechanisms of color reactions used in NCR Paper, and the further implications of such mechanisms. For example, the possibility of producing color changes in materials by ferroelectric-like shifts in structure has been discussed. A prospective employee was invited to one of the evening discussions. At the conclusion of the meeting, his comment was, "This is what I've been looking for all my life." He is now a highly successful member of our team.

Educated Thinking

During the development of NCR Paper, C. F. Kettering was a member of the N.C.R. Board of Directors. Many of our

research people had an opportunity to observe the "Kettering Philosophy" of scientific agnosticism and healthy disregard for the pat truisms of textbooks. Oftentimes, Kettering gave timely moral support to research men who seemed to be in the "left field" of "educated learning"—the men who questioned textbook validity. We were asking ourselves why the thin organic films required for complete liquid encapsulation could not be made in an automated process despite textbook admonishments to the contrary. This agnostic approach, if it may be so labeled, paid off, and today we are using organic films that are 1,000 times thinner than those formerly thought necessary to serve as solvent, vapor, and oil barriers. This habit of thinking is instilled into many of our research people and has aided us in solving many "paradoxical" problems.

Classifications and definitions are artificial and man-made, and are designed to aid in clear thinking; but they can also be a block to clear thinking. Classifications and definitions identify pin points in a vast, single field of science. It is necessary to attempt to grasp underlying truths and to avoid too literal an acceptance of authoritarian precepts.

Rapid Communications

A technique that is important in the over-all environmental picture is that of giving the research man the opportunity to personally demonstrate key advances to all supervisory personnel. This is time well spent by supervision. It aids in keeping such personnel posted on important advances and also aids in giving promotions and merit increases, it gives the researcher satisfying recognition for his efforts, and, finally, it provides rapid communications and understanding between management and research personnel. This can be done regardless of the size of an organization. It is one of the important functions of Management to give time for this purpose. It is certainly one of the least begrudged duties, and our only wish is that progress should be so significant

and rapid that even more time could be devoted to this pleasant pastime. This technique was very instrumental in gaining Management support for the long and trying years required to solve the basic problems associated with NCR Paper. This and other more prosaic techniques of communications, such as weekly progress reports, wide report distribution, and weekly research meetings, enabled Management to maintain very close contact with progress throughout the NCR Paper project.

Part III: How Top Management Views Creative Techniques

Top Management's attitude toward the creative techniques discussed is strictly dependent upon the results obtained. To Management, the techniques are simply tools with which to fashion an expanding enterprise.

It is not likely—and certainly was not the case with the NCR Paper development—that Management can remain aloof from active participation in the Research program and yet provide the integrative leadership that stems from judgment, insight, and maturity. The techniques we have discussed have in large part provided the handles which opened doors to laboratories and made it possible for Management to see into the technical workshop. By the same token, the Researcher can "look out" and understand the motivating factors in Management's "Creative Thinking."

In this paper we have used NCR Paper as "A Case History," showing how the creative techniques used have assisted in producing a practical product. There is an interesting sequel to the Case History in terms of new developments that have become possible through use of the basic chemical and physical tools derived.

After NCR Paper became established, it was possible to publish certain findings in scientific journals and discuss with other scientists many interesting facets of our work in the fields of colloid chemistry, physical chemistry, and organic synthesis. Applications of the new encapsulation process to the varied areas of indus-

trial technology which we had visualized during the development phase, now began to materialize. The close working relationship established between Research Management and Research personnel effected a kind of brainstorming that equated scientific principles and product possibilities through these channels. Suggestions have flowed freely between Top Management and Research personnel.

The basic work on color has opened new possibilities in chemical switching techniques, nonmechanical photo-printing, photocopying, and photo-recording papers. As an example, we have developed photochromic compounds which are capable of being converted to the colored form by exposure to a short wavelength light source and then converted back to the colorless form by exposure to long wavelength light. This phenomenon can be used in many combinations to make possible many diversified uses.

The microcapsules themselves are unique tools for producing many new products. Some of them are very obvious adaptations of existing systems, others are "blue sky" applications.

To illustrate the point, consider the capsules as a device for making liquid systems solid. This means we can encapsulate many foodstuffs, thus providing greater ease in handling In many instances the capsule wall itself can provide greater stability to the product. In this category come vegetable oils, liquid and solid shortenings, butter, food flavors, cream, etc.

Perfumes are volatile and expensive—seemingly the more expensive, the more fugitive they are. But again, the capsule is a way of making the perfume nonvolatile until the dry capsule is deliberately broken, thus releasing the unharmed perfume to perform its desired effect. Or going a step further, the permeability of the capsule may be controlled so that there is a slow leakage of perfume. Coated on tissue this would be an excellent application for the product.

The stability furnished by the capsule wall is now being investigated in regard to the protection it might afford to light- and air-sensitive vitamins and other medicinals. Considerable wastage

in drugs results today because of the loss of activity occurring during storage.

In the same area of pharmaceuticals, controlled rates of reaction are also highly desirable. It is perhaps possible that delayed reactions can be provided through careful control of the capsule wall in the structure.

One application of the capsular technique has resulted in a product which even baffles the researcher. In this case we have encapsulated magnetic pigments suspended in an oil inside the capsule. The pigment is still very sensitive to the effect of a magnetic field. This may well prove to be a new powerful high-speed printing device and it also appears promising as a high-speed light shutter.

We have not overlooked the application of microcapsules to the photographic field. The importance of this area to high-speed printing is well known, and the capsule system offers the possibility of providing very simple and rapid development and fixing. The application of N.C.R. capsule structures in the field of color photography undoubtedly suggests itself to many readers.

Adhesives that aren't sticky may well be coming out of the capsule investigations. Stick adhesives would be very convenient to use. There is also the possibility of dry pressure-sensitive adhesive tape. Or, by different modifications, new heat-seal coatings may well be forthcoming.

These few examples are illustrative of the manner in which idea growth has been stimulated in a Research environment that deliberately fosters a sense of urgency and a concern over the future. In this environment Research personnel—Management and laboratory—examine daily problems with a common perspective.

Now, at the expense of repetition, we would like to highlight several of the powerful, yet simple approaches that were used in this development to bring Research people and Management together in a unified working team:

(*1*) *Problem Orientation*, or a problem well analyzed is a problem half solved.

(*2*) *Research Referees*, or bringing objective evaluation to the subjective scientists.

(*3*) *Progress Evaluation*, or how to make "what? how? when?" reasonable questions.

(*4*) *Working Combinations*, or the team approach is the best approach.

(*5*) *Communication*, or getting the picture without a coax cable.

(*6*) *Open Project Time*, or doing what you want to when you want to.

These techniques are not theoretical but are the results of many approaches toward a practical and workable operation. When functioning smoothly, the system can be of immense value to Management. Not only does it enable Management to keep abreast of current developments, but it also helps them see the significance of the developments as they can be applied to fields not associated with present product lines.

THE X-RAY IMAGE AMPLIFIER. A DEVELOPMENTAL RESEARCH PROJECT

J. W. COLTMAN

Manager, Electronics and Nuclear Physics Department, Westinghouse Research Laboratories, Pittsburgh, Pennsylvania

There are many different kinds of research, and many definitions and distinctions which attempt to categorize them. The program I am about to describe turned out to embrace a rather wide variety of types of research. It will suffice to say that it was characterized at the beginning by two facts: first, it was set up to accomplish the invention of a device whose function was specified in advance; and second, there existed only a hazy conception of the form which the device might take.

The stimulus, and I might add a considerable portion of the cash, for this undertaking came from our X-Ray Division. This division of the company has long been engaged in the design, manufacture, and sale of x-ray equipment, largely for medical radiology.

One of the standard items of diagnostic x-ray equipment is the fluoroscope. In its simplest form it consists merely of a source of x-rays disposed opposite a screen of fluorescent material. A body interposed between the two will cast shadows from its denser portions. You will probably recollect that it was this very phenomenon that comprised the discovery of x-rays by Wilhelm Roentgen

in 1895, followed almost immediately by the introduction of the method into medical practice. While there have since been improvements in efficiency, and great elaboration of mechanical motion, the ordinary fluoroscope of today retains the major fault of its predecessors—the picture simply isn't bright enough.

Most of you have probably been subjected to a fluoroscopic examination, and have noted the complete darkness of the room and the period of accommodation required by the radiologist, but I doubt if many of you have had the experience of trying to see what the doctor is looking at. If you had, you would have been struck by the extreme dimness of the picture. Its brightness, in the case of the average patient, is only one-thousandth of a foot-lambert, some 30,000 times dimmer than the picture on a good television set. For heavy patients, the brightness may decrease further by a factor of 10 to 50.

We might well ask at this point why fluoroscopy is an important technique, and why it is difficult to make a brighter picture. It is indeed quite possible to obtain, by photographic techniques, far superior still pictures, or radiographs, than those displayed on a fluoroscope. But these pictures fail to provide one feature which may be essential in many cases—the ability to observe motion in the subject matter, either natural or induced. Few of you would attempt to diagnose the ills of an electrical appliance or an automobile engine from a few still photographs. You find it highly desirable to be able to poke, pry, and observe what happens. In the same sense, the radiologist often finds it essential to palpate the patient, and in spite of its obvious limitations, the fluoroscope has remained a primary diagnostic instrument. The matter of the dim picture is a direct result of the fact that x-rays have a deleterious effect on body tissue. One can only afford a certain dose rate at the point where the x-rays enter the patient. Now the absorption of x-rays in tissues follows, in general, an exponential law, and the quantity of x-rays emerging may, in the case of thick sections, be less than a thousandth of that incident. We are forced then to work with what emerges without increasing the amount we put in.

This is quite sufficient to obtain a good single exposure for a radiograph, but when we call for a fluoroscopic examination which may last several minutes the input x-ray intensity must, of necessity, be held to a very low value.

I will turn now to a description of the research and development project which we organized to find a practical means for providing the radiologist with a fluoroscopic picture of greatly increased brightness. It started with the appointment of the X-Ray Research Committee, a group comprised of members from the X-Ray Division, the Lamp Division, which was engaged in the manufacture of x-ray tubes, and the Research Laboratories. This committee was charged with the task of recommending and following interdivision programs which would have an influence on the course of the x-ray business of the future. Most members were from the engineering and scientific ranks; there were, however, two persons from the sales organization. This committee, which is still functioning, not only initiated the project after taking a careful look at the needs of the x-ray business, but had a profound influence on the course of the work and on the outcome. Composed mostly of people close to or on the working level, it functioned with high effectiveness, not only in this but in a number of other matters, and I can state that it provided a highly satisfactory method of operation.

As in the case of many, though perhaps not enough, research projects, the task was begun by a survey of the present state of knowledge, an outlining of the questions which had to be answered, and a set of suggested possible methods of attack. Dr. R. C. Mason, then head of our Physics Department, undertook this work, and turned out what we have referred to ever since as "The Mason Report," which in many respects is a model of its kind. The study first inquired into the x-ray energy available and into the efficiency of the fluorescent screen which converts this energy into light. This efficiency turned out to be of the order of 10%, so that while there was certainly room for improvement, there was no hope, by working on the screen alone, of achieving the very large brightness

gains desired. It was evident that some form of amplifier was required, that is, energy from some external source would have to come into play somewhere in the system. These studies also led Mason to the realization of an important fact, one which had been overlooked until this time. This concerned the quantum nature of the x-rays, which come in discrete packets of energy much larger than those of light. Thus the x-ray image itself has a graininess which limits the detail one can see even if the limitations of the eye are removed. Mason estimated that this quantum graininess might become visible with a brightness gain of 10 times and that a gain of 100 times might be all that was worthwhile. The report concluded with several suggestions for accomplishing amplification, with an estimate of the feasibility of each.

At this point there was sufficient encouragement to make it worthwhile to engage in further research and experimentation. Accordingly, a small group was formed in the Research Laboratories for this purpose. While the immediate task of this group was to pursue the problem of x-ray image amplification, it was understood that theirs was likely to be a function of long duration, that research in support of the X-Ray Division should be a continuing affair, and that considerable breadth was desirable in the work which they would undertake. I think that this was an important aspect of the setup. This relative freedom of approach, which did not narrowly define the path to be taken or circumscribe too tightly the area to be covered, had some very valuable results. First, it enabled the group to be staffed with scientists of high skills and potential who could be assured of the opportunity to carry out research representing real contributions independent of the eventual success of the main goal. Second, it enabled us to do a very thorough job in investigating not only the limits set by nature on what we could do, but also permitted us to explore some of the profitable bypaths opened up. Third, it was educational. As you are doubtless aware, pure and applied research is largely a process of learning by doing. In this broad approach, I am sure that we contributed more to the education of those members of the labora-

tory associated with the project than a more highly directed effort could have provided. There was, in fact, one man who obtained his Ph.D. degree with a thesis performed in direct conjunction with the project effort.

The make-up of this group may be of interest. To begin with, there was only one man with direct x-ray training, and that in crystallography. There were also an astronomer, a nuclear physicist, an electrical engineer, and a solid state physicist. Some of these had had a considerable background in microwave research during the war.

The first work consisted of a more direct investigation of the questions raised by Mason, particularly that of the quantum graininess. By imitating on a cathode ray screen the picture one might expect from an ideal high-gain image amplifier, and making some experiments on pattern recognition, we were able to demonstrate that the quantum grain limitation lay much further away than the first estimates, and that x-ray image amplification could indeed be very worthwhile. Incidentally, these experiments were, much later, extended and elaborated upon. The results are of considerable practical interest in pattern recognition for radar and surveillance, and the problem is one which is still being pursued by workers in psychology.

An investigation of the properties of fluorescent screens, and then of the combination of fluorescent screens and photoelectric surfaces, led to the discovery that such a combination was capable of detecting x-ray quanta as single events. Thus was born the scintillation counter, which has since essentially replaced the Geiger counter as a detector in nuclear physics research, medical radioisotope application, and uranium prospecting.

These examples may suffice to point out the kind of approach we took. It was not a necessary one; I cannot in fact say that we had by this means made more certain our chance of success nor made the pay-off date any closer. I do think that in terms of satisfaction to the research worker, in unexpected returns from some of the by-paths, in the depth and breadth of understanding which we ac-

quired, and in the skills and interests developed among our personnel it was worthwhile.

Right from the beginning, of course, we were thinking about possible mechanisms for achieving the desired amplification. Chief among the specifications was the gain; we figured that we should strive for an amplification of at least 100 times. The resolving power requirements were fairly high, considerably in excess

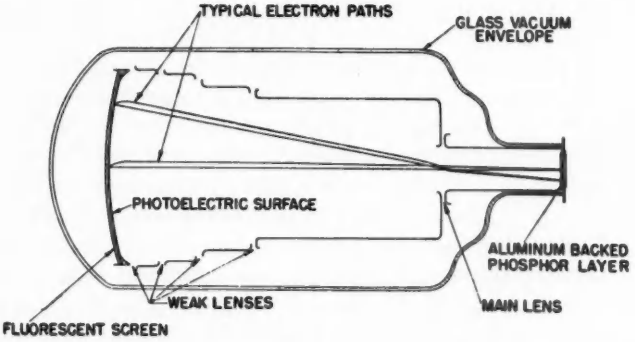

Fig. 1. Diagram of the Westinghouse x-ray image amplifier, Fluorex.

of standard television, for instance. Picture size and convenience of operation were also factors to be seriously considered. By the time some of the exploratory research was done, we had turned over in our minds several alternatives, and had, by analysis, discussion, and some experimentation, fixed upon a particular line of attack.

The method chosen was in one sense not original; it had been suggested in part in the literature, but to my knowledge, its operation had never been thoroughly analyzed nor had it been tried. As in the case of the airplane, the problem was not to invent it, but to make it fly.

The principle of the Fluorex, as the Westinghouse x-ray image amplifier is known, is diagrammed in Figure 1. The x-ray shadow is cast in the usual fashion on a fluorescent layer which is deposited

on the curved input surface of the tube. Adjacent to this layer is a thin transparent photoelectric surface. Light from the fluorescent layer ejects electrons from the photosurface, which are accelerated by potentials applied to the various electrodes of the electron optical system, and focused on a second fluorescent layer at the output end of the tube. Energy is put into the system through this externally applied potential, and by careful choice of the initial and final phosphor layers, and of the photoelectric material, a brightness gain of ten or more can be achieved.

A second and quite distinct form of gain was achieved by making the output image much smaller than the input. Since all of the electrons are collected from the input, concentrating them at the output raised the brightness inversely with the areas, so that a 5 to 1 reduction gives a further gain of 25 times. Now it is a remarkable fact that magnification of the image by an external optical system can be achieved without diminishing the brightness, and it becomes possible to obtain a final virtual image the same size as the original but 250 times brighter.

I shall not recite all of the problems which were met and overcome during the period of transforming these concepts into reality. There were made during this period, however, some decisions on questions which frequently arise during the course of such developments, on which I should like to make a few remarks.

The first was a decision to make pilot tubes rather than to jump into the design of a final model. These tubes had active surfaces only an inch in diameter, were magnetically focused so that they contained no electron optical system, and were intended to provide a simple means whereby we could directly test our ability to make photosurfaces and phosphor screens and test the brightness gains. The making of such preliminary models is a common practice in developments of this sort, and it may usually be counted upon to provide a safe and not too costly procedure for a logical development. There are some dangers involved, however, and in this case we were indeed led astray in one matter. This concerned the mutual interaction of two of the materials used

in the fabrication of the tube. In order to obtain maximum gain, the output phosphor was chosen to be zinc sulfide. This phosphor was very susceptible to attack by the cesium vapor used in activating the photosurface, and our pilot tubes turned out to be essentially inoperative on this account. There followed a rather lengthy program to find a cure for this difficulty, which included a search for more resistant phosphors, the investigation of techniques for coating the phosphor layer with protective materials, and the development of processing methods which would restrict the amounts of cesium vapor reaching the phosphor layer. I must report, however, that these labors turned out to have been quite unnecessary—in the final model the phosphor layer was so far away from the photosurface, and the blackening material used on the inner surfaces of the lenses was such an effective absorber of cesium, that the problem of phosphor attack simply disappeared. As a matter of hindsight, I can say now that we would probably have been time ahead had we omitted this step. The lesson to be learned from this is not an unequivocal one, because circumstances will surely vary widely with different projects; I can only say that making a model is not the same as making the full scale device, and that the safe and sure approach may be fraught with as many dangers as plunging directly into the problem.

However wise or unwise this decision may have been, we did achieve pilot tubes which demonstrated brightness gain with appropriate resolving power, and we had well in hand, through other work carried on concurrently, the design of a full-scale device in which we had considerable confidence.

There arose now another question which commonly emerges during the course of developmental projects: Should we keep quiet about it or should we publicize our progress? The urge to talk about what one is doing is common to individuals and corporations alike. For the individual it can be a source of gratification which is of high personal value. For the corporation, it contributes to prestige in general, helps customer relations, and may favorably influence those persons which represent the future market. It

may also provide a means of assessing the commercial possibilities, and it certainly stimulates those involved in the work to more intensive effort. To delay an announcement too long exposes one to the risk of being scooped by the competition, putting one in an uncomfortable "me too" position at a later date. On the other hand, it is quite probable that an announcement will make the other companies in the field aware of possibilities they had neglected and stimulate them to a competitive effort. If too long a time elapses between the publicity and a marketable apparatus, customer relations may well be damaged. These are rather delicate matters to be weighed, and the question concerns business acumen more than anything else.

In our case, it was decided at the time when we had just completed the pilot tube to make an announcement of our progress and plans to the public in general, but particularly to the radiologists for whom the apparatus was intended. The response to this publicity was very great, and if we had had any doubts as to the interest such a machine would evoke, they were certainly dispelled at this point. Now, of course, the heat was on; we not only had in a sense incurred an obligation to our clientele, but we had a limited time in which to keep ahead of competition.

The period between this time and the first actual accomplishment of our goal was one of intensive activity, both at the Research Laboratories and at the X-Ray Division. The latter had taken the responsibility for the design of the optical system—an important and difficult task—and all of the mounting and packaging of the tube in such a manner as to fit it best into the uses the radiologist would make of it. All our efforts were now devoted to the making of a working model which would satisfy the requirements set forth. There were problems of vacuum envelope and electron lens fabrication, of getting the input and output phosphor screens sufficiently well resolving, of forming a high quality photosurface, and of avoiding field emission. The first few completed tubes were essentially failures; not until we had been through the process several times did we obtain sufficient mastery of the techniques to

produce a workable tube with real gain and capable of giving a respectable image at the intended levels of x-ray excitation. Now the X-Ray Division had something to work with, and together we evolved two prototype units for field tests. At this point, the Research Laboratories might well have considered their major task finished. As so often happens, however, the skills, know-how, and equipment that were developed along with the device itself existed only among this particular group. Up to this time, we were not sure it could be done; now that we knew it could, it was necessary to transfer this knowledge to a production operation and at the same time not cut off the small supply of tubes that the X-Ray Division needed for further trials and equipment design. Accordingly, we set up a small pilot manufacturing operation at Research, brought an engineer from the Electronic Tube Division into the group, and turned ourselves into an engineering and production outfit. At the same time, duplicate facilities were being set up in Bloomfield. Over a period of many months, we produced a small number of good tubes, meanwhile transferring various parts of the operation to the Bloomfield plants. This transition period was a long one, and toward the end of it most of our work was devoted to ironing out many of the production difficulties encountered. It is significant to note that the quality of the tubes improved as the rate of production was increased—it is typical of devices of this sort, which are essentially handmade items, that specialization of the operators and of the equipment used tends toward greater uniformity and better performance as the skills of the operators become more highly developed.

There are many parts of the story of bringing the Fluorex from its conception to actual use in the field which have been omitted or only lightly touched upon—the lengthy process of evolving a design best suited to the doctor's use, the sales and marketing effort for a completely new device, and problems of keeping up production quality and delivery, in all of which Research was called upon to play some part. I will conclude by taking a brief look at what some of the results of this effort were.

We have now in use in hospitals and other radiological centers some 250 units, which are doing a fine job and have received much praise from their users. Pathological conditions have been observed which were heretofore quite invisible, either fluoroscopically or with radiography. The certainty and ease with which certain delicate operations can be performed has been greatly enhanced, and the radiation dose given to patients during examination markedly reduced. We have established the X-Ray Division as a forward-looking outfit, and the name of Westinghouse has gained a good deal of respect among our customers. We did, incidentally, get our money back; not lavishly, but certainly in sufficient amounts to justify the very sizable investment we made. The unit has been adapted for taking motion pictures, which heretofore had been extremely difficult, and the X-Ray Division now has another product totally new in the x-ray field.

But more than this happened. The skills and know-how introduced into our Electronic Tube Division have led them into the business of producing orthicon and vidicon camera tubes for television. At the Research Laboratories, the personnel engaged in this work turned their skills and facilities toward further researches in the basic phenomena of photoemission, electron-induced conductivity, and other topics in electron physics. Out of these, in turn, are coming some very interesting developments in a variety of imaging devices, with applications ranging from astronomy through commercial television and military electronics.

All in all, the creation of this small group to tackle a particular problem has had a rather far-reaching influence on many of the Company's activities. I believe this has occurred for three reasons; first, the group was part of a large organization which could take advantage of any of a number of possible results, second, they maintained intimate contact with the effort in the operating divisions, and, third, they took a rather broad interpretation of the task which was set before them.

I would like to conclude by remarking on one aspect of the project which had a not inconsiderable effect on those who were

engaged in it. This was the essentially humanitarian end use for the product. In these days of emphasis on military weapons and the somewhat mixed blessings which science and technology have brought to our modern civilization, it is gratifying to know that the efforts one is putting forth are unquestionably directed toward the advance rather than the retrogression of mankind. To the Westinghouse Corporation, the Research Laboratories, and the individuals concerned with the project, this has been a source of great satisfaction.

EXPERIMENTAL DESIGN: A TECHNIQUE FOR GETTING MOST INFORMATION AT LEAST COST

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This is a short chapter from the research career of Joe Doaks—somewhat as an Horatio Alger story might have been written.

Joe was a good student in a small Midwestern high school and had a B+ average for four years of college where he majored in chemistry. Due to the influence of a stimulating professor of organic chemistry, he decided to go on to graduate school and was successful in obtaining admission to one of the better ones. He did his thesis work in low molecular weight synthetic organic and took a Ph.D. in due course. He was well regarded by the professor under whom he did his research.

Joe accepted a position with the ABC Corporation and was assigned to work on high polymers, an assignment he thought he would like.

Joe had never heard of systematic experimental design methods.

The particular genes which are responsible for keen scientific intuition had largely been left out of Joe, so that when he got into the intricacies of high-polymer research he rapidly got into hot water. There were so many variables involved in high-polymer synthesis, testing results were far from being error-free, and con-

clusions drawn from one set of experiments didn't seem to hold when different experimental conditions had to be used.

Joe wandered around in this maze for quite a while getting more and more unhappy—and so was his supervisor whenever he thought about Joe's work.

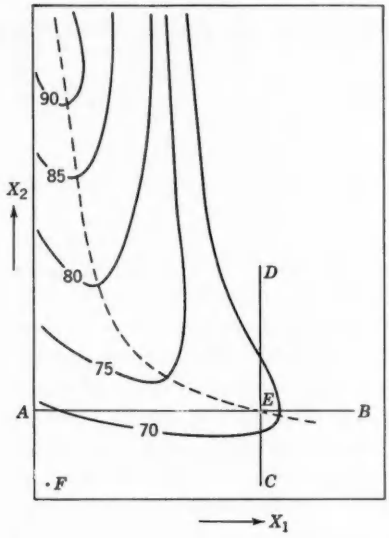


Fig. 1. Effect of experimental conditions on yield in a chemical synthesis.

Some other men hired about the same time seemed to have a built-in intuitive sense that got them through relatively simple experimental mazes without much trouble, and it began to look to the Research Director as if the company had done fairly well with that year's crop of new chemists but had missed the boat when they hired Joe.

One day Joe saw a review of a new book which was entitled, *Experimental Design*. By this time he was willing to try anything,

and although the company library wasn't getting the book, he ordered it himself.

Wading through it was tough going. It presupposed a knowledge of such things as sampling theory, variance and covariance analysis, correlation, matrix algebra—and Joe had always more-or-less subconsciously taken the position, to paraphrase an old joke, that "us organic chemists don't need no advanced mathematics." But he was now almost desperate, so he got through it.

Briefly, it gave him a substitute for his lack of intuition in simple systems, and ultimately, when he had mastered some of its techniques, a tool more powerful than even fine intuition when applied to a complex system of many variables.

As he began to use these procedures more and more, he began to get meaningful results, and, to his delight, he found he could express the degree of confidence he had in a result by an actual number. His supervisor, who was always more or less under pressure budgetwise, found that Joe was suddenly getting more information at less cost. He too was pleased!

Joe finally got a raise.

One of the first hints Joe had as to the cause of some of his trouble was a figure in the book which is reproduced here as Figure 1. (Based on Davies, *Design and Analysis of Industrial Experiments*, Oliver and Boyd, London, 1954, p. 504.)

This is a sort of contour map showing the effect of two independent variables (which might be temperature and pressure) on the yield in a chemical synthesis. In this map yield corresponds to altitude in a geographical contour map, so that points of equal yield are connected by the curves. One would call this a rising curved ridge. It is not uncommon in chemical systems.

Let us suppose that Joe had been assigned the problem of studying the synthesis with the object of finding the conditions of maximum yield, knowing nothing, of course, about the shape of the surface.

In his early days he would have used the usual or classical approach to such problems which might be illustrated as follows:

He would first hold pressure constant at the value or level represented, say, by the point A and would have carried out a series of experiments at different temperatures, which means that he would

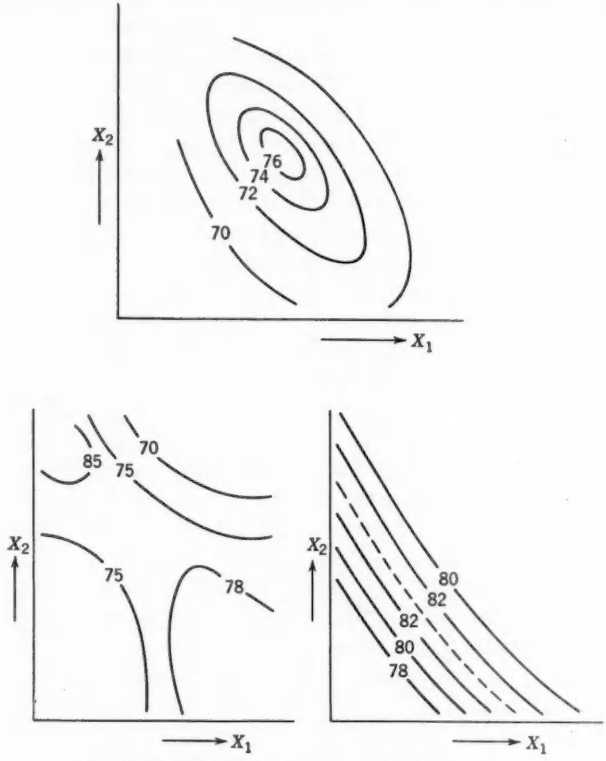


Fig. 2. Typical contour maps in chemical systems.

have been following along the line AB. By this procedure he would have found his maximum yield at E. He would then have held temperature constant at the level which gave him this yield, and would have carried out a series of experiments at different

pressures above and below the level *A*, following along the line *CD*. It is obvious that he would have concluded that the maximum possible yield was at point *E*, a little over 70%.

If, however, he had had the intuition, as some of his colleagues might have had, of starting out at or near the point *F*, and first carrying out a series of experiments at different increasing pressures, he would ultimately have hit the 90% yield-peak jack-pot which was there to be found—but only if varying pressures were studied first.

This was just about the first constructive thing he had run across. A little further search showed him that there are many sorts of curved contour maps which are fairly characteristic of chemical systems, three of which are shown in Figure 2. (Based on Davies, *Design and Analysis of Industrial Experiments*, Oliver and Boyd, London, 1954, p. 504.)

After a study of these and other similar figures Joe came to the following conclusions:

1. "When I attack any problem involving two or more independent variables, I will almost never have any idea of the shape of the contour map representing the effect of these variables on the property I am interested in, which I shall call the response. Consequently, I shall drop the words 'contour map' and adopt the term 'response surface' (which is used generally by the statistician) to describe a plot of the results."

2. "I will define the classical approach as that method of experimentation which involves varying one independent variable, while holding all others constant, until I have found the optimum response for that variable, then holding it there while varying one of the others until I have found its optimum, and continuing this process until I have found the optimum for all by this successive method."

3. "The more sharply curved the real but unknown response surface of the system I am investigating, the less chance I will have, on the average, of finding the over-all optimum in the surface by this classical approach."

It took only a few such pictures to convince Joe that a great deal of his trouble was due to the fact that he had been using the classical approach on sharply curved response surfaces, and was therefore frequently missing the optima by large margins.

But what could he do about it?

It seemed obvious that somehow the entire surface had to be investigated if he was to be reasonably certain of finding the highest peak or ridge, or possibly the deepest hole or valley. If he was investigating only 2 variables, he could pin a picture of the coordinates of the surface against the wall and shoot a charge of bird shot at it, carrying out an experiment at each shot hole. Regardless of the obvious disadvantage from the standpoint of the annoyance of the owner of the wall, this still seemed a crude method, and Joe reasoned that there must be a more systematic method of making sure he wouldn't miss a peak as he'd been doing, and which would be applicable to many variables.

And thereby, after more study, he discovered what is known as the factorial method of experimental design, which had been so useful to others in the same boat.

To show how it works, I propose to describe the case history of one of Joe's later researches. He informs me that the results are now under study by the ABC Corporation's patent lawyers, and consequently the material worked on, the meaning of the variables, and the name of the tests used cannot be disclosed. Since this is a known idiosyncracy of all patent attorneys, I am forced to bow to it and will have to describe the work in terms of *X*'s and *Y*'s, Test *A*, *B*, etc. The usefulness to you, I hope, will not be impaired.

The Problem

Joe and his group (by this time he was a group leader!) had developed a new continuous chemical method for making a new product and had put together the pilot-scale machinery and equipment. There were 3 quantitative variables involved in the

equipment which I shall have to call X_1, X_2, and X_3, although you wouldn't be far wrong if you assumed that X_1 and X_2 were time and temperature. It was possible to carry out the preparation in 1 of 2 different media or solvents, so the effect of these 3 variables had to be separately investigated for each solvent.

The resulting product was to be put through 9 different laboratory tests, all presumably unrelated. In certain of these minimum values were desired, in others maximum values.

The problem was to select the values of X_1, X_2, and X_3, for each solvent, which would give the "best" or "optimum" compromise over all the 9 tests, assuming each test to have equal importance in the final decision.

Preliminary experimentation, as well as theoretical considerations, indicated that some or all of the response surfaces should be sharply curved—hence the classical method would be useless.

Joe actually was working against time. Somebody had leaked to the sales department, the whole project looked hot to them, and the usual pressure that sales departments know so well how to apply had been applied.

Joe figured he could afford the time to carry out only about 30 experiments on each solvent for a total of 60 in all, and he therefore had to have the best design he could get within those limits, which would at least show up the major curvatures and their directions. He selected a $3 \times 3 \times 3$ factorial design.

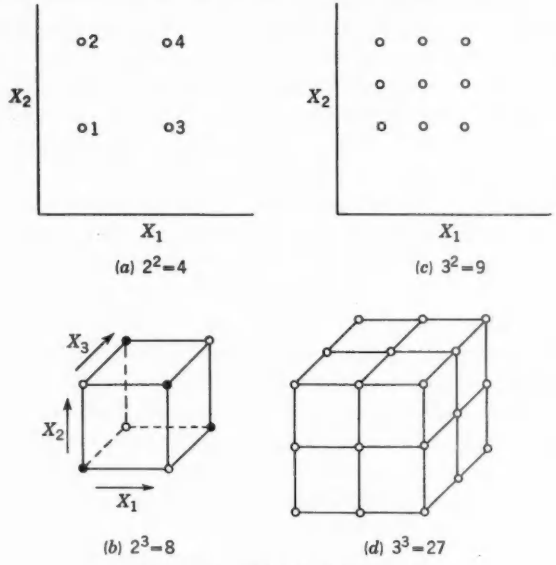
The Nature of the Factorial

Joe's explanation of the nature of a factorial design and why he used the 3^3 is about as follows:

"The primary purpose is to spot the experiments over the possible or practicable experimental region in such a way that as much of the true nature of the response surface can be determined as we have the time or money for. Obviously, with only a few experiments, only a hazy idea of the nature of this surface is obtain-

able, and the more experiments we can afford to carry out the better is our estimate of its true character.

"A second purpose is to use such a system of scattering these experimental points as will minimize subsequent calculations while maximizing the amount of information obtained.



(a) $2^2=4$ (c) $3^2=9$

(b) $2^3=8$ (d) $3^3=27$

Fig. 3. Several factorial designs.

"The first step in laying out such a design is, then, to lay out or imagine the coordinates of the system, where each axis represents one of the independent variables—time, temperature, pressure, or whatever they may be.

"Figure 3 shows several such designs. Look first at Figure 3*a*, which is a simple 2 × 2 laid out between the coordinates X_1 and X_2, which again might be time and temperature. It is apparent that we are studying 2 levels of time at 1 temperature, and the same 2 levels of time at another temperature.

"If we add a third independent variable, X_3, say pressure, we get Figure 3*b*. This merely adds another dimension to the 2 × 2 and results in a cube involving 8 experiments instead of a square. Please neglect for the moment the difference between the blackened and unblackened circles.

"If instead of 2 levels of each variable we wish to investigate 3 levels, we get Figure 3*c* for 2 variables and 3D for 3. The only limitation on this addition of a third level is that the intervals between the levels along any axis must be equal.

"Do not forget that these diagrams are merely pictures of the experimental conditions. Each point represents a particular combination of these conditions, which we have called the independent variables. When the experiments are all complete and the yields or any other desired response have been measured, a plot of that response against the experimental conditions adds another dimension to the diagrams in Figure 3. For example, if we had carried out a 3 × 3 factorial on the synthesis represented in Figure 1 and plotted yield against the 2 variables of pressure and temperature, we would have gotten a three-dimensional plot or contour map something like Figure 1, and some of the curvature of that figure would have been shown up, depending on where the experimental points had been located.

"If, on the other hand, we had carried out a 2 × 2 factorial, then again depending on where we had located it, upward or downward trends would have been evident, but very little curvature since only 2 points in each direction would have been used.

"Obviously then, if we are interested only in major trends or main effects and care nothing about curvature, 2 levels are sufficient. If we wish to get an estimate of curvature, 3 or more levels must be used.

"We can fit these points into an equation, if we wish, by a method called the least-squares procedure and come out with a polynomial equation which empirically represents the entire surface. From this equation any of the characteristics of the surface can be calculated, such, for example, as the expected yield for any

combination of pressure and temperature in Figure 1. It is easy to see that once such a polynomial equation is available, calculations can then be made, if desired, to determine the position of any maximum or minimum, that is, the particular settings of temperature and pressure which will result in these characteristics."

Joe tells me that the preceding explanation is somewhat oversimplified and that a certain amount of important detail has been left out, but that the essence of the method is there in as simple language as he knows how to put it.

The Method of Attack

In Joe's case the response surface was thought to be curved. He therefore had to use at least a 3-level factorial to measure this curvature. Since he had 3 independent variables, the simplest design he could use was the 3^3 giving 27 experiments, and this was the design selected, as shown in Figure 3*d*.

If you will remember Joe's first reaction to the contour map—that of scattering his experimental points over the surface with a shot gun, you will see that he has scattered them all right by this design, but by a systematic method instead of by the random method.

Actually, the random shot gun method would probably work equally well if he used bird shot and a choke bore at 20 or 30 feet, but the later computing work would be much more expensive and complicated.

In essence then, the factorial design gives as complete coverage of the entire experimental region (the statistician calls it the factor space) as one can afford in time or money, and minimum computing cost.

It is, of course, usually much safer to interpolate than to extrapolate. Accordingly, since the practical limits of the 3 machine variables were known, and nothing was known of the nature of the response surface other than its expected curvature,

Joe selected these limit values as the extreme or corner points in his design. The third point for each variable obviously was then placed half-way between these limits. Joe recognized that this fairly wide scatter over the entire possible experimental region, while it would give him a good measure of any smooth regular curvature of his response surface, might nevertheless fail to pick up any sharp peaks or valleys. The only way to catch these would be to use more levels for each variable, for which he didn't have the time. Actually, there were other ways, one of which will be discussed later, but it involved sacrificing certain information as to the interrelation or interactions between variables, and he felt a knowledge of these interactions was so important that he daren't sacrifice them for more complete coverage.

Having set up the design and selected the experimental points, the order in which the separate experiments were to be carried out was randomized so as to prevent some systematic time effect, such as ambient weather conditions, operator psychology cycle, or other unknown effect, from introducing a systematic error. The experiments were then carried out, and the analytical data obtained.

The data so obtained were then fed into a digital computer from which resulted, for each of the 9 tests, a least-squares polynomial equation which empirically represented the response surface for that test.

When Joe had these 9 polynomial equations available for each solvent, he calculated the values of X_1, X_2, and X_3 from each to give optimum values for the 9 responses. Fortunately, with one exception, all were optimized at closely similar settings so that final selection of the best operating conditions was merely a matter of picking the mean. Of course, if some of the tests had been more important than others, he would have had to use an appropriately weighted mean.

Thirty-six pilot plant runs at these settings and with the best solvent were then made in order to produce a quantity of the product for field experimentation. Joe has kindly supplied me with the mean values for all the 9 tests for these 36 runs, and I have

listed them in Table I as compared with the values predicted from the polynomials.

TABLE I

Agreement between Predicted and Found Values

Tests	Average of 36 pilot plant runs, P	Predicted from polynomials, N	Difference, P − N	Difference as a % of P
A	2.59	2.68	−0.09	− 3.5
B	2.69	2.74	−0.05	− 1.9
C	0.10	0.06	+0.04	+40.0
D	2.43	2.30	+0.13	+ 5.3
E	0.51	0.59	−0.08	−15.6
F	0.58	0.76	−0.18	−31.0
G	3.52	3.65	−0.13	− 3.7
H	1.58	1.63	−0.05	− 3.2
I	0.52	0.37	+0.15	+28.8
Totals	14.52	14.78	−0.26	
Means	1.61	1.64	−0.03	− 1.79

You will note that only where numerically small analytical values were obtained were the deviations between predicted and observed values large percentage-wise, which suggests a fairly constant numerical error in each analytical test. The over-all means were very satisfactorily in agreement. The relatively small numerical values of the deviations indicated that the computed polynomials were reasonably good approximations of the true response surfaces.

The Cost Comparison

Now as regards cost, which is a word appearing in the title of this paper so it's probably about time I said something about it. The 54 experiments Joe and his group had run took 2 weeks. His group's cost, including salaries, overheads, etc., but not including

equipment, was about $4,000 per month, the digital computer programming and machine time was $650, so the total cost of the designed experiment was about $2,650. Joe estimated that if the classical approach had been used, and taking account of the reasonably large experimental error which would necessitate replication of each experiment, somewhere between 150 and 200 experiments would have been required to reach as good an estimate of the optimum conditions. He also states that without intuiton it might have required many more. This number of experiments would have cost between $6,000 and $8,000, so that the saving to the company was between $3,350 and $5,350. If Joe performed this well on all projects he would have gotten two to three times as much useful information per year as the worker using the classical approach in complicated problems.

In addition, because of the sharp curvature of some of his response surfaces, the classical method would have been unlikely to have given as good a result without exhaustive work.

A Standardized Design Approach to All Problems

Joe, of course, is nobody's fool—now—and he has realized that to perform this well in all experiments takes some kind of a systematic approach. Some of his work involves a search for new and better high polymers, and this necessitates preparing them in the first place and insuring that the analytical and test results are sound. He has developed a simple system which is based on the following facts.

1. Preparation

In any high-polymer preparation there are several independent variables; some quantitative, some qualitative. Among the quantitative variables may be temperature, possibly pressure, reaction time, per cent conversion (although the latter two may be correlated), catalyst concentration, emulsifier concentration, etc.

Among the qualitative variables may be type of solvent, type of catalyst, type of emulsifier, type of reactor, etc.

2. Testing

If the product is tested in any sort of multiple-position machine, say for abrasion resistance, there may be a significant difference between positions or between runs.

His reasoning from these facts runs somewhat as follows: In view of the strong probability that any such multiple-variable response surface is usually more-or-less curved, any experimental combination chosen at random may easily fall in a region of poor physical properties whereas some other combination might conceivably give a product of better properties than any now known.

Since the object of this type of research is to find a better polymer than anything now known, it is pretty much a waste of time and the company's money to make a single preparation, test it, and abandon it if the tests are poor. Conversely, it may be an equal waste of time to carry out an extensive factorial experiment on every new idea in order to insure that any possible superior combination is not missed.

Some sort of compromise is needed for the initial screening.

1. Preparation

Joe found what he considers as the desired compromise in what has come to be known as the Box method, after the statistician G. E. B. Box who was largely instrumental in its development, although certain other investigators, among them Hotelling, had laid the ground work.

If we will look back for a moment to Figure 3*a* we can get at the essence of the method. This gives the design of a 2^2 factorial experiment. If Joe has a 2-variable experiment to conduct, he always carries out such a 2^2 factorial in order to observe if there are any upward or downward trends along either ordinate. He locates

this factorial within the region in which he expects such trends will occur, and if he gets an upward trend, even though the properties of the product are relatively poor, he moves out in the direction of the trend or trends and repeats the experiments. If he gets a downward trend he moves back in a direction away from the trend.

Sharp trends in the direction he wants to go indicate he may have something worth following up. Poor results and the absence of any appreciable trend suggest that the experiment may be dropped before much time and money are wasted, unless *a priori* reasons cast doubt on this conclusion. In this case a second factorial some distance away might be justified.

This is all very simple and straightforward so long as the preparation contains only 2 independent variables. When there are more variables, as there usually are, even the 2-level factorial can involve a great deal of work. For example, with 8 variables the 2-level factorial involves 2^8 or 256 separate experiments.

Obviously, for these multi-variable preparations, some further compromise is needed, and this Joe has found in what is known as the fractional factorial.

The fractional factorial is a portion of the full factorial that has been divided up in a certain specific way to meet definite requirements. If done correctly it often permits a clear measure of the main linear trends or effects of each variable on the response, but a complete knowledge of the interaction is sacrificed. At the present stage of the game Joe is only looking for main trends, so a knowledge of the interaction doesn't matter. Later, however, such a knowledge will be necessary, especially if the new polymer looks good enough to justify finding the optimum conditions of preparation.

It looks then as if this would be a good place to explain the meaning of the term interaction and what it signifies to Joe in his work.

To keep it as simple as possible, reference is made again to Figure 3*a*, which shows the experiments involved in a 2-variable, 2-level factorial. If the response measured, say for example impact

resistance, shows an upward trend in going from point *1* to point *2*, but a different upward trend in going from point *3* to point *4*, then an interaction exists. This is shown more clearly in Figure 4 where the impact resistance is plotted against X_2. It is seen that the curves have different slopes. The statistician would say that if this difference in slope is real, and not caused by error in the measurement, then "there is a significant interaction of variable X_1 on variable X_2."

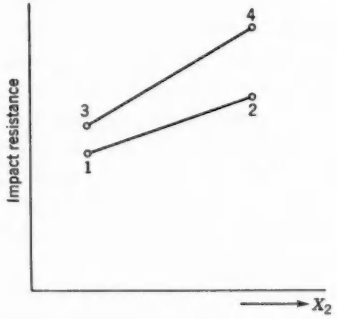


Fig. 4. Illustration of an interaction.

To indicate how measures of this property are confused in fractional factorials, reference is made to Figure 3*b* which shows the experiments involved in a 2-level, 3-variable factorial. The blackened circles represent the half-factorial. After a little study one would conclude that the main effects can be calculated, but they are confused with all the 2-factor interactions, and the design is useful for indicating main trends only when there is good reason to believe that the interactions are negligible. If this assumption cannot be made, the full factorial of 8 experiments must be run.

Contrasted with this the 8-variable preparation mentioned previously, which involved 256 experiments, can be studied as a $^1/_{16}$ fractional factorial of 16 experiments only and all main effects

will be clear of 2-factor interactions, but all interactions are confused with each other.

These interactions show up in the product terms of the polynomial when such a computation is made from a full factorial but can also be computed by a simpler method if the full polynomial is not desired.

Where large desirable trends have been noted by the use of some kind of fractional factorial design, the next step is to find the optimum in the system. The procedure used here may be one of two. A large factorial or fractional factorial can be set up to cover the entire experimental region such as the one Joe used in the case history described, and there are star designs which can be superimposed on the factorial which are a sort of combination of the full and the fractional. These are generally used when subsequent testing may be long and drawn out and the testing results cannot be obtained quickly.

The other procedure is to continue to repeat the small fractional factorial in the direction of the trends, thereby following one's nose up-hill until the highest region is reached. This procedure is obviously worth doing only if the testing results can be obtained quickly, for the levels of the variables for each experiment can be chosen only after the trends from the last experiment are known. It suffers from the disadvantage that if there is more than one peak or ridge in the response surface, then once started up-hill only one of them may be discovered and it might not be the highest "optimum." The full or star factorial covering the entire experimental region is the safer but may be much more costly. Decision as to which to use must be made in terms of the benefit to be derived by finding the highest peak or lowest valley.

2. Testing

Joe tells me that one of the troubles of his earlier days was the unreliability of certain test results, particularly those from highly accelerated tests of some time-dependent deterioration such as

abrasion resistance. These tests are ordinarily run in a multiple-place machine and he has given me an interesting little story of some of the problems involved.

Table II gives the results of two tests of abrasion on four samples: *A*, *B*, *C*, and *D*. It doesn't even take much intuition to see that while *A* is higher, no differences between *B*, *C*, and *D* can be demonstrated.

TABLE II
Wear Ratings

Sample	Test I	Test II	Average	Rank
A	268	251	260	1
B	218	241	230	
C	235	229	232	2
D	236	227	232	

Joe wanted to find out whether any differences between position in the machine or between separate runs could be found, and if so, whether differences between samples *B*, *C*, and *D* could be demonstrated by making allowance for such machine error. You can easily see that his screening method, using the 2-level fractional factorial, is no good whatever if no differences between the responses of the separate experiments can be demonstrated because of testing error.

He reasoned that if somehow he could set up an experiment in which each sample was run once in each position, he could estimate the difference between positions. Similarly if all samples were included in each run, he could estimate the difference between runs.

Table III shows such an experiment, which the statistician would call a Latin Square. Every sample has been run once and only once in each machine position. Since there were 4 positions and 4 samples, this obviously took 4 runs. You will note that Run *3* gave an unusually high value as did position *2*.

TABLE III

*The 4 × 4 Latin Square**

Run	Position in the machine				Average
	4	*2*	*1*	*3*	
2	*A*251	*B*241	*D*227	*C*229	237
3	*D*234	*C*273	*A*274	*B*226	252
1	*C*235	*D*236	*B*218	*A*268	239
4	*B*195	*A*270	*C*230	*D*225	230
Average	229	255	237	237	

* See Davies, *Design and Analysis of Industrial Experiments*, Oliver and Boyd, London, 1954, p. 164.

Using these data, and with a certain amount of statistical legerdemain, he was able to eliminate the position and run variability and compute a range for each sample within which the odds were 19 to 1 that the true value lay.

These ranges and the conclusions he was able to draw from them are shown in Table IV. You will note that now samples *A*, *B*, and *C* can be distinguished from each other, leaving only sample *D*, which overlaps *B* and *C*, intermediate in rank.

TABLE IV

Sample	Mean value	Range	Rank
A	266	256–276	1
B	220	210–230	3
C	242	232–252	2
D	231	221–241	?

This experiment pointed up the importance of position and run variability and indicated to Joe that where the machine involved was known to show these two sources of error to any appreciable extent, either the machine needed improving or a Latin Square should be carried out whenever any of his Box-design samples were under test.

Author's Note

This is a brief summary of certain techniques which have been found eminently useful in industrial experimentation. There are many more, and it would take books to describe them all. It seems unfortunate that so many of our graduate schools are turning out Ph.D.'s in Physics and Chemistry with so little knowledge of these time-saving and cost-cutting methods.

All sorts of specialized methods are being developed within the general statistical framework, of which life testing, where only a few units from a shipment can be tested, is an important one. The trend also seems to be in the direction of development of theoretical knowledge using the empirical polynomial as a starting point, a procedure which makes any necessary extrapolation somewhat more safe, as well as advancing the particular science involved.

Joe's one disadvantage is that he is not a statistician, does not have ready access to one in his company, and generally tends to use somewhat more clumsy or antiquated designs than those appearing in the recent literature of statistical design.

But he at least has proved to himself the value of these methods I have described, and some of his colleagues are getting interested, since it seems to pay off—in time, money, and salary raises. In fact, as of the present writing, a full-time statistician is being sought who can supply the necessary help to the entire department.

INFORMATION RESEARCH

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The purpose of this paper is to describe how we in the Chemical Research Department at Atlas Powder Company have formalized and are starting to make good use of a function and research tool that in one way or another has been with us for a long time but on occasion has not been employed too effectively.

I imagine that everyone has his own idea of what Information Research might be. What it is and what it does goes by several names. Some people call it Literature Research or Abstracts and Searches, others call it Technical Information, others don't call it anything but realize the necessity for a technical staff to have the proper background knowledge before proceeding with research planning and experimental investigation.

The very extensive Technical Information program of the Esso Research and Engineering Company carried out by its Technical Information Division contains what I mean by Information Research but is of much broader scope.

The function of the Technical Information Center of Hercules Powder Company at their Experimental Station also embodies Information Research but again is much broader than that of the Information Branch of our Chemical Research Department.

We have a research program of broad aims and objectives made up of research projects of greater specificity. Each project in turn consists of a number of areas, subjects, or phases of in-

vestigation. We also have a variety of skills and abilities among our research people, and one of our main jobs is to focus the right talent at the right time on our projects. I can imagine some of you saying to yourself "Why that is only what every research director does every day." I mention it because I want to emphasize the fact that research men use tools in order to do their research jobs effectively and creatively, and since some people use one kind of tool better than another, it naturally follows that the proper disposition of people and skills is bound up with the tools they use. It seems to me there are two kinds of research tool; first a tangible one measured by the huge investment in laboratory buildings, equipment, supplies, and service presently considered necessary to support the experimental activities of a research man, and second, an intangible one, consisting of technical information, both internal and external to the company, selected and considered in the light of the company's research objectives, so as to provide support for the mental and creative activities of the research man. All of the available skills and tools ought to be brought to bear on the problem at hand in the most effective manner possible. Both kinds of tools are necessary.

In the past many companies have assumed that each research man will—as it is sometimes stated—"keep abreast of the literature," and accordingly they provide a technical library and hope that all will go well. It is also hoped or assumed that the information amassed by company research in the past, and dutifully reported in technical reports—bound copies to be found in the library—will be sufficiently well known to all the research men to be of use on current endeavors. This works reasonably well when the research group is small, because there aren't very many projects, and the kind of work doesn't change much and everyone knows what everyone else is doing anyway.

The picture is not the same, however, when the technical organization grows larger and more complex. Communication becomes more tenuous, specialization becomes evident, and responsibility becomes divided, not shared. The informational basis

for planning a basic research program is different from that required for the most effective support of a specific project. Likewise the background of technical information needed for the planning of an application research program is different from that needed for the conduct of a specific application or development project. To rely on the individual efforts of laboratory scientists to provide the intangible research tool—information—for both program planning and individual projects, does not appear to be the answer to the research director's job of focusing the right talent at the right time on the jobs in hand.

Information Research at Atlas is represented by the Information Branch of the Chemical Research Department. I believe it will be clearer why we have set it up as we have, and what it does, if I tell you a little about the organization of the Atlas research and development functions, which have undergone considerable change during the last few years.

Our company has two operating divisions, Explosives and Chemicals, and several corporate staff departments. The Explosives Division maintains its own facilities for explosives research, development, and technical service activities, which are mainly concerned with fairly specific and short range objectives. The Chemicals Division also maintains its own Product Development Department and Chemical and Engineering Department for product and process development, applications, and market research and technical service. These groups are concerned with specific short-range (1–3 years) objectives in the area of the Chemicals Division's present line of business.

The Chemical Research Department is a corporate staff function and its activities involve broader and longer range objectives. In the program of the Chemical Research Department are included basic research projects, pioneering, exploration of areas of chemistry new to Atlas, improvement in basic technology or processes, discovery of new reactions and products, and further utilization of raw materials and technology available to Atlas. The successful output of the Chemical Research Department

comprises (*1*) technological information generally useful in the technical areas of our business, (*2*) a demonstration that specific goals of products, processes, or uses are realizable with further effort, and (*3*) the selection of areas of new endeavor for company growth. We also carry out research projects sponsored by Explosives Research, Product Development, or Chemical Engineering, in areas where our skills, facilities, or experience warrant.

Our internal organization is as follows: Dr. Walter Rueggeberg is Director of the Department. Under him are three section managers, Dr. J. D. Brandner, who is in charge of the Physical-Chemical and Analytical Section, Dr. J. W. LeMaistre, in charge of the Organic Section, and I, who am in charge of the Administrative and Technical Services Section. Included in this latter section are budgetary and fiscal matters, laboratory services, the company technical library, and the Information Branch. We consider that the Physical-Chemical and Analytical section, the Organic section, and the Information Branch make up our research team or laboratory staff and are responsible for carrying out our research projects.

Now we come back to the point of skills and tools. The men in the Organic, Physical-Chemical, and Analytical groups are laboratory men for the most part and "make with the test tubes," so to speak. The men in the Information Branch work in offices and the library and "make with the books." All of them are research workers, however, and contribute their respective efforts directly to assigned projects.

The staff of our Information Branch presently consists of 7 professionals and 2 clerks. The supervisor, Dr. Fred Benson, has a Ph.D. in organic chemistry, and 16 years experience in chemical research, including 4 years at Picatinny Arsenal. Dr. Benson came to Atlas a year ago from Metalectro Corporation, now a division of National Distillers, as the nucleus of our group. The rest of the professionals includes 3 organic Ph.D.'s with industrial research experience ranging from 5 to 8 years and including some teaching, 2 organic Masters, one with 6 years and the other with

1 year of industrial research experience, and 1 woman, having a B.S. degree in chemistry, a Master's degree in Library Science, and 4 years' experience at Atlas, of which 3 years have been as assistant librarian and the last year as a chemist in the Information Branch.

Briefly, here is what this group does:

To provide background data for present and projected research, the Information Branch makes literature surveys on topics pertinent to corporate research, as well as on topics requested by the Product Development Department and the Explosives Division Research Department as sponsored research. The end results of such surveys are topical reports. Less frequently, for information requests of a fragmentary nature, accumulated data may be presented orally or made the subjects of memoranda. In addition, the current chemical literature is surveyed on a planned basis. The results of this are made available to Atlas technical personnel by means of a semi-monthly Abstract Bulletin and, in some instance, by memoranda. In connection with both of the previous functions and, in some cases, independently, ideas of possible value to Atlas are generated, including patentable concepts.

The effort expended on surveys approaches 85% of the time of the members of the Information Branch. The surveys have, on the average, required 4 months for completion. This is somewhat longer than originally anticipated and is presumably dependent on the nature of the subject matter of the surveys undertaken and also the factor of comparative inexperience on the part of most of the members of the group in this type of work. It is anticipated that the average time required per survey will be brought down to 2 or 3 months in subsequent operations. I would like to point out that besides the factual information and bibliography in each report, recommendations for specific action are included; these recommendations are based upon the information in the survey. This latter practice differs from many information organizations in other companies, where frequently only known or established facts are presented, but not the results of creative effort or competent judgment. The recommendations made in the survey

reports represent the same kind of critical evaluation, imagination, and awareness of project objectives as a chemist at the bench or his supervisor employs in assessing experimental progress.

With respect to the perusal of the current literature, at present each professional member of the Information Branch nominally spends 15% of his time on this function. Editorial, make-up, and related functions for the Abstract Bulletin require more effort—approaching 35% of *one person's* time. The Abstract Bulletin contains about 50 to 70 items per issue, and an attempt is made to achieve a balance of reader interest with respect to organic and physical chemistry, fundamental and applied chemistry, processes, products, and uses. Our reading public consists of research and development people, mostly in chemical or explosives research and product development with some in chemical engineering. Our present circulation list numbers 85.

The selection of items for abstracting involves judgment of the pertinence of the item to our present research and development interests and the likelihood of stimulating thinking in areas vaguely contemplated but not as yet part of our committed research and development effort. A few items of general interest relating to procedures or experimental techniques are also included. Only bona-fide technical articles are abstracted. Trade news is not included.

Where appropriate, comments are made to place the abstract in context with other information or with Atlas current interests. Sometimes it is possible to make provocative comments to stimulate thinking outside of current channels. To do this, the personnel of the Information Branch have to be thoroughly familiar with the research programs of the several research and development groups, and keep well informed of the current status by perusal of their progress reports. We feel that the comments have value because they bring a fresh viewpoint to the research man concentrating on his own problems.

The device used to keep the Abstract Bulletin within the arbitrarily prescribed time limits is to restrict the number of journals abstracted. At the present time, 34 journals are being examined.

These vary from weekly to bi-monthly publications, but most are monthlies. The abstracting currently is being performed by all of the 7 professionals. The selection of the journals to be abstracted has involved a balance between journals of a more fundamental nature (such as *Journal of the American Chemical Society*) and applied journals (such as *Journal of Agricultural and Food Chemistry* or *Journal of Petroleum Technology*). The principle has been followed that for each fundamental journal added to the list, one applied journal also should be added. American, British, and German patents are being abstracted also; the abstracts being made from the patents themselves, rather than from the *Official Gazette* or the data in Derwent's *British Patents Report* or Derwent's *German Patents Report*. Far more patents are ordered (particularly U.S.) than are actually abstracted, since frequently insufficient information is available in the title and the one claim published in the *Official Gazette* adequately to decide about a given patent's applicability to Atlas' interests. Although it is believed that the current list of journals being abstracted is a desirable one, there are many important journals not being covered.

At this stage of the game, the right number of people in our Information group is pretty much of a guess, and we see no simple and absolute answer to the problem. We had originally budgeted 5 or possibly 6 for this year, and find ourselves with 7 as the result of a fortunate circumstance. Many of you are aware that we are building a new laboratory to house our Chemical Research and Product Development Departments and plan on increasing our laboratory staff over the next year or two.

At the present time there isn't any more laboratory bench space left, but we still have some office and library space where newly employed researchers can be happily and profitably employed on Information Research until the new laboratory is finished. We have three newly acquired men in this category, but can count on them only until the new laboratory is ready, sometime after the beginning of the year. So actually we have only 4 permanent members of the projected 5 or 6 in our Information Branch.

Next year we hope to have 6 professionals doing Information research out of a total of 53. We are figuring, as a tentative future goal, to have about one-sixth of the professional laboratory staff assigned to the Information Branch.

We are still feeling our way with our Information Research group, since it is only a little over a year old, but the group and its personnel are gaining considerable respect from our other technical departments.

The 3 men presently on loan to us until the new laboratory is ready have taken readily to their temporary assignment and are acquiring a good orientation to our research program and over-all technical activities. It is serving as a training course. Because of this we are toying with the idea of rotating research personnel both to and from the Information Branch and the laboratory sections, on a planned and continuing basis, but maintaining a more or less permanent nucleus. It is believed that the tenure within the Information Branch should be a minimum of 6 months. During that time the "temporary" employee would, in addition to working on one or more surveys, also function on the Abstract Bulletin, which should aid in overcoming any tendency to over-specialization, and would allow an atmosphere conducive to generation of new ideas. It is believed that about 4 professionals should constitute the permanent staff and the remainder be in the rotation schedule.

The selection of personnel for the rotation schedule could be made on one or both of the following bases:

1. Analogous to the procedure formerly employed by many chemical industries whereby chemists were first placed in analytical service, it would seem that there is some advantage in placing personnel first in the Information Branch.

2. When a new major project is to be started, one of the professional personnel to be assigned this project in the laboratory would do well to spend a period of time in the Information Branch first. Thus, the necessary literature survey for the future operations would be carried out under conditions more conducive to a thoroughness in the literature survey than would be the case if an

individual is nominally functioning in the laboratory. The pressures for immediate action would be lessened by virtue of the chemist being placed in the Information Branch temporarily.

There are two other activities often associated with a technical information group that we are not doing at present.

One of these is the deep indexing of internal research reports. This activity is, with some companies, the major function of their information organizations. While this probably should be a future function of the Information Branch at Atlas, for the immediate future no activity along this line is planned. A necessary prelude to such indexing would be to determine the subject areas to be covered for maximum utility to Atlas research, the depth necessary, and the optimum degree of mechanization. Indexing can be costly and the benefits to be derived should be carefully balanced against the time and effort required. The Library, at this time, is indexing our technical reports, and it seems unwise, now, to obtain sufficient additional Information Branch personnel to carry out this indexing function on a more thorough scale. The indexing activity requires a somewhat different approach and, to a degree, a different type of personnel than those currently in the Information Branch. It is our present feeling that it would be a better policy to start the indexing function at a later period when the demand for long and extensive surveys has been reduced somewhat.

One other possible function that has been suggested for the Information Branch is that of performing translations. Again, this is, in some information organizations, a major activity. Since the primary function of the Atlas Information Branch is to furnish technical information, together with deductions and inductions derived therefrom, the need of the Information Branch for translations is in the nature of a tool. Generally speaking, our group is competent in French and German, and essential translations of individual articles from Russian (or other language) can be obtained by purchase from outside organizations. This practice is currently being followed by the Information Branch, as necessary.

Several sources are available to learn of articles of interest from language sources other than German, French, and English. First is *Chemical Abstracts*, the biggest objection to which is the time lag from initial publication. Second is the *Current Chemical Papers* list published by the Chemical Society. We plan to use this source extensively. Besides this, commercial organizations, such as Consultants Bureau, Inc., publish tables of contents of certain Russian journals and translations of articles therefrom. None of the foregoing is intended to imply that additional language skills, such as Russian or Italian, would not be useful in the Information Branch. The relative advantages of technical chemical competence and language skills, however, are all in favor of chemical competence. It is unlikely (but not impossible) that suitable personnel will be found possessing both the necessary chemical background and abilities in other languages such as Russian, Italian, and Dutch.

To sum up, our Information Branch is primarily a research rather than a service group. Its members do literature research as necessary, direct contributions to our research program, and create new ideas. It also provides stimulation to the rest of our technical personnel via the Abstract Bulletin, which calls attention to specific items in relation to Atlas interest. It utilizes an organized group skill to provide literature surveys. It is a training ground for new men, for by exposing them to areas of Atlas research and development interest, it allows them to acquire familiarity with projects and program before plunging into experimental laboratory work.

It does not, however, replace individual literature searches by a laboratory man. It is not a specific information retrieval section. It is not a general technical information disseminating or routing bureau.

It is truly a part of research, and functions in an atmosphere highly conducive to creativity. Although information research at Atlas has been formally in being for only a year, we are pleased with what it has done and expect greater things from it in the future.